dissertation, ETS. Ing Telecomunicación, Madrid, 1973.
[3] S. Yoshizawa, "Some properties of randomly connected networks of neuron-like elements with refractory period," *Kybernetik*, vol. 16, pp. 173–182, 1974.
[4] S. Amari, "A method of statistical neurodynamics," *Kybernetik*, vol. 14, pp. 201–215, 1974.

Comments on "A Perceptual Channel for Information Transfer over Kilometer Distances: Historical Perspective and Recent Research"

LEON D. HARMON

There are many ways in which to comment upon the above paper.[1] I imagine that PROCEEDINGS readers will provide a fine variety of feedback. My own reaction, however, is very simple.

If one takes the trouble to categorize roughly the contents of this ramble, the sequence looks like this:

Topic	Number of Pages
1) Introduction	1
2) Informal anecdotal assertions	$1\frac{1}{2}$
3) Historical background	$3\frac{1}{4}$
4) Main study: Introduction, procedures, controls	$\frac{3}{4}$
5) Results, gratuitous editorial comments, anecdotal procedure citation	$5\frac{1}{4}$
6) Informal presentations and discussions of further experiments	10
7) Analysis	$1\frac{3}{4}$
8) Raw data transcript	1
9) Bibliography	$1\frac{1}{2}$

The central issue in a deeply controversial and highly suspect topic such as telepathy, clairvoyance, time reversal, etc., is whether one is prepared to accept as true what is offered in evidence. Notice that in the rough categorization of the article's contents, above, *only three-quarters of one page* (p. 335) in a 26 page paper is concerned with the critical issue of rigorous experimental protocols and controls. And much of that slim section is cursory and anecdotal.

We can keep our eyes on the ball by examining *solely* the relevant three paragraphs on page 335 of the Puthoff and Targ paper (par. 3, 4, 5). All the rest—background, anecdotes, drawings, discussion, and other (less formal) experiments, delightful as they may be—can be set aside while we peer closely at what must ultimately supply reasonable satisfaction regarding credibility.

The signal-to-noise ratio of this article improves markedly when 26 pages of meander are replaced by three paragraphs of explicit relatively formal description of experimental procedure.

We are told the following.

1) The experiment was double blind.
2) The "transmission" experimenters were given "target" locations and proceeded to the target while the "reception" experimenter was kept ignorant of the target.
3) Experimenters were with the "transmitting" subject at all times during the "transmission."
4) An experimenter was with the "receiving" subject at all times during data taking.

The entire business now hinges on the reader's accepting on faith that no information was transmitted conventionally at any time from, say, the transmission experimenters to the receiving experimenter or to the subject. But no controls are cited; no safeguards are described; no neutral watchdogs are mentioned.

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The author is with the Department of Biomedical Engineering, Case Western Reserve University, Cleveland, OH 44106.
[1] H. E. Puthoff and R. Targ, *Proc. IEEE*, vol. 64, pp. 329–354, Mar. 1976.

Further, this was *not* a true double-blind experiment as claimed, since at least one of the researchers had *a priori* knowledge of the presumed occult data. We are asked to believe that no conceivable communication channel existed between "transmitter" and "receiver" other than by some exotic attenuationless seemingly magical information propagation.

I feel certain that your readers can conceive of many possible alternative conventional channels. Both electronics engineers and magicians, for example, will be at no loss to suggest many.

It is comforting to know that the Editor and Reviewers of the PROCEEDINGS recommended publication of this preposterous material. At least they are open-minded—which is a good thing. But then, too much open-mindedness is a hole in the head.

Reply[2] *by Harold E. Puthoff and Russell Targ*[3]

We would like to comment on the points raised in Harmon's letter in response to our article.

In his introductory remarks, Harmon makes a reference to "less formal" experiments. It is important to state at the outset that there were no such experiments. In every experimental series, from the Costa Rica pilot study to the verification study with visiting government scientists, and resolution study with technology targets, a rigid formal protocol was followed. This required that the experimenter with the subject always be kept ignorant of the chosen target and that the analysis (judging) of the experiment be done in a blind fashion by an individual who did not know which response was associated with which target.

Harmon suggests that the reader of our paper must accept *on faith* that there was no conventional communication channel from the target site to the subject, since "no controls are cited, no safeguards are described, no neutral watchdogs are mentioned." In fact, if Harmon will examine p. 335, he will find that the entire experiment had multiple controls, safeguards, and watchdogs every step along the way.

With regard to control over target selection at the beginning of the experiment:

> Before the experimental series began, the Director of the Information Science and Engineering Division, not otherwise associated with the experiment, established the set of locations as the target pool which remained known only to him. The target locations were printed on cards sealed in envelopes and kept in the SRI Division office safe. They were available only with the personal assistance of the Division Director who issued a single random-number selected target card that constituted the traveling orders for that experiment .... The experimenter remaining with the subject at SRI was kept ignorant of both the particular target and the target pool so as to eliminate the possibility of cueing, overt or subliminal ....

When it came to the departure of the target team, an experimenter plus one to three "watchdogs" assigned by SRI management were handed the travel orders, left SRI, got into an automobile, opened the orders, and then proceeded to the site indicated. As stated in the paper, "The target demarcation team, consisting of two to four SRI experimenters, then proceeded by automobile directly to the target without any communication with the subject or experimenter remaining behind." We find it remarkable for Harmon to read that the target demarcation team consisted of two to four experimenters, and yet argue that perhaps they were not vigilant with regard to the possibility that one of their members might try to communicate back to the subject. In addition, numerous of these experiments were observed by visiting government scientists, outside consultants, SRI management, etc. The roles of the two main experimenters were often reversed as to who remained with the subject and who accompanied the outbound team. The composition of the outbound team was changed, and many times did not include either of the main experimenters. In short, as stated on p. 335:

> At all times, we and others responsible for the overall program took measures to prevent sensory leakage and subliminal cueing and to prevent deception, whether intentional or unintentional. To ensure evaluations independent of belief structures of both experimenters and judges, all experiments were carried out under a protocol ... in which target selection at the beginning of ex-

[2] Manuscript received April 9, 1976.
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periments and blind judging of results at the end of experiments were handled independently of the researchers engaged in carrying out the experiments.

Harmon goes on to comment that "this was not a true double-blind experiment as claimed, since at least one of the researchers had *a priori* knowledge of the presumed occult data." This is in error. Neither the experimenter with the subject nor the judge who evaluated the data, the two individuals in principle capable of affecting the outcome, had any knowledge of the true correspondences, and thus the experiment *was* double blind. The fact that experimenters sent to the target site knew where they were is irrelevant, since they did not have any opportunity to interact with subjects or judges.

With regard to the mechanisms involved in remote viewing, Harmon states, "We are asked to believe that no conceivable communication channel existed between 'transmitter' and 'receiver' other than by some exotic attenuationless seemingly magical information propagation." We wish to indicate to Harmon that the two mechanisms discussed by the authors, extremely low frequency (ELF) electromagnetic propagation and quantum correlation, though they may seem exotic and magical to him, are well understood phenomena in the engineering and scientific community, both theoretically and experimentally.

Finally, as we indicated in some detail on pp. 340–343, the watchdog aspect was carried to the extreme, by having visiting government scientists who were interested in observing our experimental protocols be subjects themselves in an effort to detect whether chicanery was involved. In the process of trying to account for their own good results on the basis of other than paranormal functioning, they expressed concern that perhaps the experimenter might be cueing them subliminally. This was countered by eliminating the inbound experimenter and having the visitor remain alone in the lab throughout the duration of the experiment. They then conjectured that perhaps after the experiment they were being taken to a place that sounded like their description, even though that may not have been the place where the outbound experimenters had gone. This was countered by having the outbound experimenters make a tape while at the site and turning it over to the subject–critic at the same time that he turned over his own tape describing the remote scene. In such fashion every criticism was met. Thus, although Harmon suggests that "both electronics engineers and magicians" will be at no loss to suggest many possible alternative conventional channels, we find that, to the contrary, both electronics engineers and professional magicians, who have consulted on this project have in fact *not* found any viable alternative to fault the SRI experiments. We therefore consider it important to continue data collection and to encourage others to do likewise.

Further Comments[4] *by Leon D. Harmon*

I was delighted to see the nature of Puthoff and Targ's response to my letter. The only rebuttal needed is to invite the reader to examine the article and both letters with care and then to judge whether or not my criticisms were responded to.

A similar example of nonresponsive obfuscation by these gentlemen can be found on pages 6 and 8 of *Scientific American* for February 1976.

I tried on two separate occasions to get permission from them to visit and see for myself, preferably with a neutral but hard-nosed observation team of my choice. The requests were met with point-blank refusal. Tch!

Further Reply[5] *by Harold E. Puthoff and Russell Targ*

We agree with Harmon that it is very desirable that interested readers examine with care the article and letters to which he refers, and come to their own conclusions with regard to the points he raises.

We understand Harmon's desire to visit SRI to "see for himself" experiments in progress. As we are sure Harmon can appreciate, he is one of more than fifty who have made similar requests in the past year. We have therefore out of necessity limited such observation to contract monitors and their consultants, potential sponsors, and researchers involved in serious attempts at replication of our work.

If Harmon is genuinely interested in determining whether the experiment works as reported, we would suggest that he try the experiment himself under his own conditions as many others have done. Such independent observations are much to be preferred, if for no other reason than on the issue that an experiment that is not replicable from lab to lab would be more of an art than a science. However, as we have indicated in our response to Harmon's first letter, it is the robustness and independence of environment or subject that characterizes this particular experiment. Therefore, although a demonstration at SRI would be satisfying to Mr. Harmon and to those who know and trust him, we think it would be a mistake for the field. That kind of experiment is basically to provide testimony, but science goes forward on the basis of independent experimentation and replication, not testimony.

[4] Manuscript received April 15, 1976.
[5] Manuscript received April 21, 1976.

Adaptive Monopulse Beamforming

LLOYD J. GRIFFITHS

Abstract—**A new receive-array adaptive beamformer configuration is presented. The array output signal consists of the difference between a conventionally weighted beam and an adaptive beam that is constrained to have a spatial null in the direction of interest. Adaptation then provides minimum total array output power.**

Adaptive receiving arrays have been extensively discussed in the literature [1]–[4] and have been shown to provide significant interference rejection properties. In most of these systems, the arrival direction and/or temporal properties of the signal of interest are assumed to be known *a priori*. If the specification of these properties is inaccurate, the actual desired signal may be treated as interference by the adaptive beamformer and thus may be rejected to some degree by the processor. For this reason, practical adaptive beamformers are generally operated in parallel with a conventionally formed array output which has fixed prespecified mainlobe and sidelobe characteristics. Comparisons between the adapted and conventional outputs can then be conducted to ensure that the desired signal-to-noise and interference ratio is indeed being enhanced by the adaptive beamformer.

The array processor suggested in this letter incorporates a conventionally weighted beam as an integral part of the total beamforming structure, as depicted in Fig. 1. In this figure, Z is used to denote the K-dimensional vector of received array-element signals and G is a fixed prefilter, which ensures that the system is steered in the direction of interest. Thus G is either a set of bulk time delays (for broad-band systems) or a network of phase shifters (for narrow-band arrays), which ensures that the desired signal portion of the K-dimensional vector X is in phase at all components. Equivalently, for a digital beamformer,

$$X(k) = s(k)\mathbf{1} + N(k) \tag{1}$$

where $s(k)$ is the kth sample of the desired signal, $\mathbf{1}$ is a constant vector of ones, and $N(k)$ is the sampled vector of noise and interference terms.

The adapted beam output signal $y_A(k)$ is formed using a system of tapped delay lines, one for each received component, as discussed in [1]–[3]. For a system with L taps per delay line, $y_A(k)$ may be expressed as

$$y_A(k) = \sum_{l=0}^{L-1} X^T(k-l)\, W_{Al}(k) \tag{2}$$

where T denotes transpose and $W_{Al}(k)$ is the lth column of delay-line coefficients employed at the kth sampling instant. A conventional output $y_C(k)$ is formed using a vector of fixed coefficients W_C, which are applied to the input data after a suitable delay, corresponding to the midpoint of the delay lines in the adaptive processor. Thus

$$y_C(k) = X^T(k-n)\, W_C \tag{3}$$

where $n = (L-1)/2$ when L is odd and $n = L/2$ or $(L/2)-1$ when L is even.

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